all the other European nations, allow me to state that Claude Bernard, the great physiologist, asked the Minister of Public Instruction, M. Bardoux, to rent four tables for French naturalists at Naples; and if this has not been achieved, there comes in a greater obstacle than national prejudice—the untimely death of the great physiologist. I believe, indeed I know, that even now a view is predominant among some of the highest authorities of the French biological school, that France ought to be represented at Naples, and it is regretted in some quarters that "national prejudice" is allowed to triumph over those higher aims of the French mind, to which science, as we all know, owes such splendid manifestations and such grand achievements.

I do not know whether it is a better position to plead for the abstinence of France in view of the several French zoological stations; but as Austria has not ceased to rent tables at Naples though in possession of a national station at Trieste, so France might have found it well worth the outlay of an annual £100 to have a share in the maintenance and profitable use of the largest and the only international biological laboratory existing.

If it be alleged that the Naples Station is now a thriving institution, and not any more in need of being supported, as in the case of the table rented by the British Association, I am glad that the author of the article in NATURE gives the account of the receipts and expenditure of the Naples Station, and finishes with the statement, that "the Station would be carried on at a considerable annual loss were it not for the magnificent subsidy of £2000 a year, granted to its support by the German Empire, which just covers the deficiency." I think this statement answers more than fully the question of the desirability of the "international" support of the Naples Station. If it were true that the Station was essentially a German institution, the German Empire would certainly not ask for the support of any other State or foreign Association, but would receive foreign naturalists as guests in a laboratory maintained for the benefit of its own subjects. But the scientific and international importance of the Naples Station is so unrestrictedly recognized at Berlin, that whilst there is a movement on foot to create in Heligoland a "Prussian" biological station for home interests, I am distinctly told that this will in no way interfere with the generous subsidy given by the German Government to the Naples Station.

I believe myself to have been the first to suggest the formation of a net of zoological stations round the globe, and have been either actively or morally helpful in the formation of most of those now existing. If I have not carried out an old plan to assist personally in the creation of a Zoological Station at Sydney, which I considered, and consider still, of the highest importance to science, it was in deference to the remonstrances of my late friend Prof. F. M. Balfour, who insisted even more than myself upon the supreme necessity of a powerful central establishment of the kind, and opposed, even for a time against my own opinion, the plan for the foundation of a British biological station, on the ground that it was too early, and would so interfere with the thorough development and maintenance of the Naples Station.

And I think I ought not to conclude without once more respectfully and gratefully recording the splendid gifts of some British naturalists, headed by the late Mr. Darwin, to the Zoological Station, which, in a dangerous moment, went far to protect my, at that time, still isolated and not generally recognized efforts from falling short of the end in view. May these two names be suffered to test the high and purely scientific character of the Naples Station, and may this reference to them help to maintain the ties which, from the beginning, have been established between it and the British biologists.

ANTON DOHRN.

THE HIGH-PRESSURE AREA OF NOVEMBER 1889 IN CENTRAL EUROPE, WITH REMARKS ON HIGH-PRESSURE AREAS IN GENERAL.

UNDER this heading Dr. Hann, of Vienna, has recently had a memoir published,[1] in which he gives in detail and discusses the meteorological conditions and circumstances in the high-pressure area which remained nearly stationary over the Alps and the circumjacent territory in November 1889, during fourteen days. On November 6 there was high pressure over the Atlantic Ocean, France, and the southern part of England. On the morning of the 11th the centre lay over the North Sea, and on the 12th it was transferred to Central Europe, and nearly the whole of Europe was comprised within the high-pressure area, which continued until the 25th. During this time there was low pressure over the extreme north-west, north, and north-east of Europe, but no distinct storm-centre up to and even beyond the 60th parallel of latitude. The centre of high pressure, 780 mm. reduced to sea-level, lay over the eastern part of the Alps. The wind, as shown by the chart, seemed to blow gently out from this centre, and at the same time to turn toward the right, indicating an anticyclonic motion. The charts also show that the region of high barometric pressure corresponded with that of low temperature, the latter, however, without any reduction to sea-level.

After reducing the pressure and temperature observations of twelve high-level stations of the Alps and adjacent territory, with altitudes ranging from 1400 to 3100 m., to the level of 2500 m. of altitude, the centre of high pressure is found to correspond, at that level, with that at the earth's surface, and the temperatures, with little variation between stations, to be a little below that of incipient freezing.

The temperature on the earth's surface first sank under the influence of the high pressure below the normal. Before this, a temperature prevailed which was considerably above the normal, which first sank to the mean on the 11th and 12th, as the centre of high pressure was first transferred to Central Europe.

The dryness of the air at the mountain stations in the centre of the region of high pressure was extraordinary during the whole time from the 12th to the beginning of the stormy west winds on the 25th, and the daily mean of the relative humidity from the 19th to the 23rd ranged from 17 per cent. on the Wendelstein (1730 m.) to 49 per cent. on the Schneeberg near Vienna (1460 m.), while on the low lands with lower temperatures the air was nearly or quite saturated with aqueous vapour. In the higher strata of the air, therefore, during the high pressure, and especially during the latter part of it, there was very great dryness, while near the earth's surface the reverse was the case.

By comparing the observations of the lower stations above 500 m. and over, from the 19th to the 23rd, with the higher ones, it was found that through a range of 2050 m. there was an increase of 0°·8 in the daily mean; but for the lower intervals of altitude, the increase of temperature with altitude, for an average range of 680 m., was 7°·1. This indicates that the air was very cold near the earth's surface only, and that in ascending it rapidly became abnormally warm, and remained so up to the level of the upper stations, and, we have reason to think, much higher. This warm and dry air came not from the south, since, at a few high stations, as Sonnblick, Schneeberg in Tyrol, and Obir, the prevailing winds were northerly. It was a real *foehn*, with its characteristics of great warmth and dryness, arising from the gradual descent of air in the interior of the high-pressure area.

The departures of barometric pressure from the normal of thirty years (1851–80) in the valley stations from the 19th to the 23rd, ranged from plus 15·2 mm. to 17·9 mm., and so there was but little difference between the several stations. For the high stations the range was from 13·1 mm. to 15·2 mm.; and so here, likewise, the variations between stations was small, and the departures differed but little on the average from those at the low stations. They were somewhat in proportion to the whole pressures at the high and low stations. This was a consequence of the almost uniform temperature from the highest of the lower stations up to the highest.

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[1] "Denkschriften der mathematisch-naturwissenschaftlichen Classe der kaiserlichen Akademie der Wissenschaften," Band lvii.

While the temperature departures on the earth's surface from the normal were about −3°, they amounted at the high stations to + 8°. The region of positive departures had a much greater vertical extent than that of the negative ones, which perhaps, on the mean, had not over 300 to 500 m. of depth. The mean temperature, therefore, in the central region of high pressure up to 3100 m. above sea-level, was warmed certainly about 6° over the normal, and as the air on the Sonnblick was 8° too warm, it is reasonable to suppose that, up to a great altitude, the air of the central region of high pressure must have had an unusually high temperature.

The limits between the warmer upper air strata and the under ones, which were relatively very cold and moist, seemed to be sharply drawn. It was principally made visible by the upper limits of the fog-formation. As this did not remain strictly at the same level, but oscillated a little vertically, places situated at about the same altitude were sometimes above and sometimes below this limit, and so were subject to very sudden and great changes of temperature. Places at all times above this limit had very dry air, and, during the day, continual sunshine, while the low stations were mostly in a very cold and moist air, with little or no sunshine. The upper warm strata floated over the cold under ones without mixing with or disturbing them.

Tables of the meteorological conditions for most of the high stations during the time of the high pressure are given, all of which show that there was an increase of temperature with increase of pressure, and that the highest temperature occurred generally soon after the time of highest pressure. A few tables for other times and places are given, which indicate the same thing. In fact, this matter has been so frequently and so ably worked up by Dr. Hann, and the results are now so well known and understood, that further researches in this direction seem almost superfluous.

The same memoir likewise gives the results of a similar investigation of a low-pressure area which lay over Central Europe, and centrally over the east side of the Alps, on October 1, 1890. The isobar of 755 mm. at this time inclosed the west part of the Mediterranean Sea and the northern part of Adria. The lowest pressures were at Vienna, Budapest, Prague, Bozen, and Riva, varying but little from 752 mm. at these places. The winds were gentle, and all Europe had rainy weather, and so the typical weather of a low-pressure area. On the mountain tops of the eastern Alps moderate and variable winds prevailed. The mountain tops were enveloped in clouds, and snow fell there; in the valleys, on both the north and the south sides of the Alps, there was rain.

The temperature departures of October 1 from the normal of three years of observations were negative at all the stations. By forming groups of stations of nearly the same altitudes, the following results were obtained:—

Altitudes (metres)	410	850	1700	2160	3100
Departures	−2°·7	−3°·3	−5°·5	−4°·8	−3°·8
Mean temperatures	8·6	6·0	0·8	−1·7	−6·5

The average departure of the whole air-column, therefore, was about −4°.

In a comparison of these temperatures with those from November 19 to 23, in order to not over-estimate the temperature in the high-pressure area the morning temperatures only of the latter were used, and thus the following results were obtained:—

Altitudes (hectometres)	5	10	15	20	25	30	35
Minimum— Oct. 1 ...	7°·9	5°·1	2°·3	−0°·6	−3°·4	−6°·2	−9°·1
Maximum— Nov. 19–23	− 2·7	+6·3	4·4	2·5	0·6	−1·3	−3·2
Max.–Min.	−10·6	+1·2	2·1	3·1	4·0	4·9	5·9

From this showing Dr. Hann concludes that at least from an altitude of 1000 m. up to 3500 m. the air in the high-pressure area was much warmer than that of the low-pressure area of October 1, although the latter was more than 1·5 months earlier.

He then estimates the mean temperatures of the air-columns up to an altitude of 3 kilometres, and obtains the following results:—

In the minimum of Oct. 1	−0°·6 C.
In the maximum of Nov. 19–23 ...	+1°·6 C.

This seems to be a very fair presentation of the matter, and really, as he thinks, an under-estimate of the difference in the two cases.

A result obtained from the discussion of the temperatures on the Sonnblick, for both high and low pressures, is here referred to, which is that cyclones of the summer half-year cause a great cooling of the air up to at least 3000 metres, and that the mean temperature of the whole air-column in a summer cyclone, from the earth's surface even up to and above 5000 metres, is lower than that in an anticyclone.

Some of the deductions from the results of the memoirs by the author, on account of his prominence as a meteorologist, require here some special notice. With reference to these results he says, "So much at least is established, that an inquiry into the cause of the cyclonal and anticyclonal motions of the air must take into account the fact *that up to at least 4 or 5 km. the mean temperature of the air in the centre of an anticyclone may be higher* (perhaps, indeed, always higher) *than that in the centre of a cyclone*."

He further says, in view of this: "Herewith fall the views which have sought the cause of these motions in the difference of specific gravity of the air in a cyclone in comparison with that of an anticyclone; in the impulse to which the air in a cyclone is said to be subject."

This seems to be said with reference to Espy's condensation theory, according to which the ascending air in a cyclone is warmer from the latent heat given out in the condensation of the aqueous vapour, and consequently lighter than the surrounding air. The argument, of which the conclusion merely is given, seems to be somewhat as follows. The temperature in the cyclone of October 1, 1889, was several degrees lower than the three years' normal, and also a little lower even than that of the anticyclone from November 19 to 23, more than a month and a half later. Also the temperatures of summer cyclones over the Alps, and a few other places, have been found to be less than the average temperature or normal. Therefore the temperature in a cyclone is less, and the specific gravity greater, than in the surrounding air at the time of the occurrence of a cyclone.

With regard to taking into account that the temperature in an anticyclone, in the sense in which it is used in the memoir, may be greater than that of a cyclone up to a considerable altitude, the writer does not see how that has anything to do with the cause of the motions of a cyclone, except, perhaps, in a few rare and special cases. During the fourteen days of the anticyclone over the Alps, even if the temperature within had been very much greater, he does not see why Espy's conditions of a cyclone could not have existed in America, on the Atlantic Ocean, or even in the north of Europe. These conditions, as is well known, are that the upper part of the atmosphere shall be cooled down below the normal, so as to give rise to a greater vertical gradient than usual, the gradient required being less for air entirely saturated, or nearly so, than for drier air. Whatever may have been the cause of the anticyclone, that of the cyclone was of course different; and in the case of the supposed cyclone in the north of Europe at the same time, there might have been a little overlapping and mingling of cause and effect on the adjacent sides, but neither cyclone nor anticyclone would have interfered materially with the other; and the complete conditions of a cyclone not being interfered with on all the other sides, these would have controlled the cyclone. Even the conditions of a small cyclone can exist in a large area of pressure considerably above the normal pressure, since it is only necessary that the vertical distribution of temperature be such that the ascending air, by its law of cooling, is kept a little warmer than the surrounding air. It is admitted that, in the case of a high-pressure area being formed and maintained for a considerable time, until the descending currents have caused an abnormal heating in the air within at some distance above the earth's surface, these conditions cannot be fulfilled.

Of course, if there was a ring of high pressure formed all around a cyclone, and maintained for some time, the *foehn* effect of the descending current would soon destroy it. A broad ring of this sort with a very moderate increase of pressure is always formed around the cyclone, but the ascending current of the interior of the cyclone comprises so small an area in comparison with the broad area all around when the air very gently settles back again towards the earth, that the *foehn* effect of the latter may be regarded as being almost insensible. So far as it goes, however, it tends to work the destruction of the cyclone, as well as the ascending currents do, by gradually decreasing the vertical temperature gradient in conveying heat from the lower to the upper strata of the atmosphere. Cyclones, in their nature, are not perpetual; for they are the means by which the atmosphere in an unstable state, which is necessary to give rise to a cyclone, is restored again to the stable state, and when this is brought about the cyclone must necessarily cease.

In the case of the anticyclone, so called, over the Alps, and all similar cases, the air comes in from a great distance on all sides, and descends over a comparatively small area towards the earth in the interior. Here the circumstances are reversed, and the *foehn* effect is very great, while the cooling effect all around, from the very slow ascent of air to compensate the downward current, is extremely small.

It is assumed by Dr. Hann that the high-pressure area over the Alps was an anticyclone, but he does not explain how an anticyclone gyration around the centre of this area could be originated, and maintained for so long a period. With an anticyclonal motion the air is pressed from all sides towards the centre by the force arising from the earth's rotation, and as the gyratory velocity below, at the earth's surface, must be diminished by friction, and this force made less there than that above, of course the descending air in the interior escapes there. If this anticyclonal gyration could be explained, it would furnish an explanation of everything else in the production of the *foehn* effect within. So far as it concerns our preceding arguments it is no matter whether it was an anticyclone or not, and want of space forbids the entering into a discussion of that subject here.

As in the case of anticyclones, so called, so it is with that of normals. They have nothing to do with the conditions of a cyclone, and the comparison of the temperature of the cyclone of October 1 with the three years' normal is not pertinent in an argument against the condensation theory of cyclones. As has been stated, this theory requires simply that the temperature of the interior of a cyclone shall be greater than that of the adjacent surrounding air at the time, without regard to what a three years' normal or any other normal may be. The three years' normal may be in error 1° or 2°, and the observed temperature may have been 5° below a true normal, and yet 5° or more above the prevailing temperature at a distance all around at the time. It is well known that the temperature departures from the normals are often very large and of long continuance. The excess of temperature in a cyclone above the surrounding temperature need not be large. If a column of air were 1° (1°·8 F.) warmer, from the earth's surface to the top, than the surrounding air, it would give rise to an ascending current at an altitude of three miles of about thirty-five miles per hour, not considering friction. Of course considerable allowance must be made for this, but, making ample allowance, there is still a velocity left much greater than that which usually exists in the ascending currents of cyclones.

It is true that the products of condensation, falling from high and cold altitudes, cool the earth's surface and the lower part of the atmosphere in summer cyclones. But this is an effect of the cyclone, and does not enter into the conditions which give rise to it. It affects surface temperatures mostly, and these are the temperatures observed in the Alps. The power of the cyclone is mostly above, in the cloud region, and surface temperatures have little or no effect. The air below in the cyclone may be colder than the surrounding air, yet, as soon as the gyration is started above, by which the pressure is decreased in the interior of the cyclone and increased around it, this lower colder air is brought into circulation by the difference of pressure thus produced, and the action upon it of that above by means of friction. But, as has been said, cyclones are not perpetual, and, so far as the cooling of the body of the air is concerned, it is one amongst others of the causes which retard and gradually destroy the motions of the cyclone after they have been once started. The principal part of the observed cooling in summer cyclones is, no doubt, due to the exclusion of the solar heat by the clouds, but this is compensated by its absorption in the upper part of the cloud.

Dr. Hann seems to have strangely overlooked the fact that cyclones do not occur in a normal, but an abnormal, state of the atmosphere; that is, when the upper strata are so cooled down as to give an unusually great vertical temperature gradient, and so an unstable state of the atmosphere. This has been illustrated in great detail by the writer elsewhere,[1] both in the case of dry and moist air. Since, then, cyclones can occur only when the upper strata are abnormally cold, and the temperature in the interior of cyclones, as we have seen, is but little above that of the surrounding adjacent air, the average temperature at high stations of a large number of cyclones must necessarily fall below the general average of temperature of the month, or season of the year, in which they occur, and in any special cases could hardly be expected to come up to this average. Not only in no special cases, therefore, but also in no cases of averages, can any argument be based upon the comparisons of cyclone temperatures at high stations with normals.

Another quotation which we make from Dr. Hann's memoir reads as follows:—"Ferrel's views with regard to the nature of anticyclones, as still maintained in his latest work, 'A Popular Treatise on the Winds' (London, 1889), are likewise in manifest opposition to the facts. A stationary anticyclone during 14 days over the whole of Central Europe, as in November 1889, and so many others of longer continuance (December 1879, January, February, 1882, &c.), cannot still be regarded as satellites or dependencies of cyclones, and support the position, 'The duration of the area of high pressure depends upon that of the cyclone' (p. 343). Rather the reverse is the case—the cyclones depend upon the anticyclones; these last rule the general character of the weather, and control the directions of the former. The anticyclones, also, do not depend directly upon the lower temperature, as Ferrel thinks (p. 344); the anticyclone of November 1889 disproves this in a striking manner, as well as the more general position, that the cause of the anticyclone is the increased density of the air through the diminution of its temperature."

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[1] "A Popular Treatise on the Winds, &c.," pp. 36 and 232.

In the first of these references to the writer's work, the anticyclone and ring of high pressure which accompanies the cyclone are spoken of. These are clearly deduced from theory, and always observed where not obscured by other irregularities. Many references could be given to show this,[1] and it is not, therefore, "in manifest opposition to the facts." It is stated that this ring of high pressure is often superimposed upon so many other irregularities that it is broken up into areas of high pressure, and a complete ring of high pressure is not observed. With regard to these it is said that their duration depends upon that of the cyclone which has caused them. Now, what argument can be drawn from the area of high pressure over the Alps, or any similar ones, against this reasoning the writer is entirely unable to see.

In forty-four cases of ridges, or areas of high barometer with an area of low barometer between them, passing over the United States, the late Prof. Loomis found the average distance from the centre of low barometer to that of the areas of high barometer preceding and following to be about 1000 miles. Now it is very reasonable to suppose—in fact, absolutely certain—that these ridges of high pressure are caused by the centrifugal forces of the gyrations of the cyclones, which drive the air from their centres and cause low pressures there, and at the same time give rise to the ridges of high pressure between them. Yet Dr. Hann would have us believe that these areas of high pressure do not depend upon the cyclones, but rather the reverse—the cyclones between are caused by the areas of high pressure.

Again, in this reference it is stated that "the anticyclones do not depend directly upon a lower temperature." In the high-pressure areas which immediately follow the great winter cyclones of the northern part of the United States and of British America, the writer took the position that the high pressure is not wholly due to dynamic causes, but in a great measure also to the lower temperature of the air on the north-west side of the cyclone, both from being brought down from a higher and colder latitude by the cyclonic gyration, and also from the increased radiation of heat into space, on account of the great clearness and dryness of this air in comparison with that of the cyclone which had just passed over. But Dr. Hann says this has nothing to do with the matter, and also that the increased high pressure in no way depends upon the cyclone; but he does not inform us what the cause of it is. A sudden change of 30° F. is no unusual change immediately after the passage of a cyclone, and if this extended to the top of the atmosphere, and without any diminution of the height of the atmosphere, this would cause an increase of nearly two inches in the barometric pressure. But it is not to be supposed that this great difference of temperature extends to the top of the atmosphere, and besides we must make some allowance for a little settling down and contraction of the height of the column, or rather disk, of cold air. But still it is very reasonable to suppose that the pressure is very much increased from the lowering of its temperature.

Of course, if this high-pressure area were a stationary one of 14 days' continuance, there would be a large body of heated air formed in its interior, commencing a little above the earth's surface and extending up to a considerable height, which would change the initial temperature conditions and tend to diminish the downward pressure, and finally to bring it to the normal pressure. This is what took place in the anticyclone, so called, over the Alps. It will be remembered that the high temperature and all the *foehn* characteristics took place from November 19 to 23, during the last part of the 14 days' continuance, and just before its sudden breaking up, which was caused by this increase of temperature within it. The vertical circulation being once established, of course it was continued by the momentum beyond the point where the temperature was equal to that of the surroundings, just as the pendulum does not stop when it has reached its lowest point, but continues on, even to the distance whence it started, if there are no frictional or other resistances. The same is true in the ascending air in a cyclone. It continues, by virtue of its momentum, not until its temperature is reduced to that of the surroundings merely, but to a still lower temperature, and until the diminished temperature and increased density gradually destroy the ascending velocity, and cause the air to be deflected laterally on all sides above. This is another reason why the observed temperature in a cyclone at high altitudes may sometimes be found to be below the normal temperature, for it may even be lower than the surrounding temperature, which, it has been shown, must itself be lower than the normal or average.

It is well known that the writer does not hold that areas of high pressure are anticyclones, and that his anticyclones surround and accompany the cyclones. It is, therefore, not logical to make observations upon one thing, as the long-continued high pressure on the Alps, and then to deduce arguments from them against something entirely different. And this is especially the case here, where it is not shown that the thing observed is an anticyclone in any sense. The writer's view is that high-pressure areas of long duration in winter depend mostly upon the diminished temperature, though dynamic causes may come in at first in the origin of the smaller ones, such as that over the Alps, already so often referred to. The areas of highest pressure and of the greatest extent are found over Europe-Asia, and only in the winter season. Of 81 cases investigated by Loomis,[1] 74 were found to have a barometric pressure of 31 inches and above, and their average extent to be 3800 miles from north to south, and about 4900 miles from west to east, the limits being determined by the isobar of 30 inches. It is unreasonable to consider these as anticyclones, and that the high pressures are due to dynamic causes. It is not only impossible to give any explanation of a supposed anticyclonal motion around so large an area, but the high pressure is very satisfactorily explained by the diminished temperature. In the northern and eastern part of Asia the normal January temperature is 45° (81° F.) below freezing; and this, from what has already been shown, is amply sufficient to give an increase of barometric pressure of more than an inch, making all the due allowances. Of course, in this high-pressure area there is a very gradual settling down of the air, and consequent increase of temperature in the lower strata of the atmosphere, a little above the earth's surface, but this is not sufficient to overcome the effect of the powerful radiation from and through the clear air during the long and clear winter nights of these high latitudes.

In the conclusion of the memoir Dr. Hann ascribes the origin of cyclones to the general circulation of the atmosphere, and considers them as being simply subordinate parts of this circulation, and independent of any local causes. It is somewhat different from the old hypothesis which prevailed before Espy's time, according to which all kinds of whirlwind phenomena originate in the meeting of counter currents of the air, and then continue often for days and weeks increasing in power and dimensions, without the apparent expenditure of any energy. He also thinks that the barometric maximums and minimums arise mostly from the descending and ascending motions of the air in the vertical circulation of the atmosphere. He says:—"There can scarcely be any longer a doubt that the pressure relations in the barometric maximums and minimums are generally explained for the most part by these kinds of motion. The forces which we really see in the atmospheric circulation in the higher latitudes, especially in winter, arise from the warmth of the tropics; that is, from the difference of temperature between the polar regions and the equatorial zone. The cyclones and anticyclones are merely subordinate parts of the general atmospheric circulation. The masses of air set in motion polewards by the upper gradients are resolved in part into great whirls, the principal progressive motion of which is controlled by the prevailing west component of the former. The influence of the inequalities of the earth's surface, the different heating and cooling of the land and ocean, and the bringing in of aqueous vapour and its condensation, come thus into account as matters of secondary importance."

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[1] "Popular Treatise on the Winds, &c.," pp. 269-270.

[1] Memoirs of the National Academy of Sciences, vol. iv. Part 2, p. 39.

Of course, nothing more can be claimed for most of this than hypothesis, but a mere hypothesis is not to be despised if it is a reasonable one. Let us now consider whether these hypotheses are of this character; and first, the hypothesis which ascribes the origin of cyclones to the general atmospheric circulation. The poleward velocities of the upper parts of the atmosphere are very small. The cirrus clouds of these regions, when not agitated by abnormal disturbances, have an apparently very gentle motion toward the east with scarcely any perceptible poleward motion. The velocity of this motion, especially in winter, may, at a maximum, amount to as much as three or four miles per hour, and that of the downward descent in the higher latitudes to a few inches per minute. Such motions are supposed to throw the atmosphere into a great whirl, several hundred miles in diameter, say in the north-west part of the United States. This whirl passes eastward, over the lakes and on to Newfoundland, with destructive and increasing violence all along its course, then passes into the Atlantic Ocean, and often into Europe, causing shipwrecks and other disasters on the way. And all this forms an incipient whirl produced by the gentle currents of the general atmospheric circulation described above. No attempt is made to show how a couple of forces could be formed from those which cause the general circulation, so as to give rise to a whirl, for this can originate in such a couple only, nor to explain where the energy comes from which keeps such a whirl in motion. It is true, Dr. Hann says the latent energy made free in condensation comes in as a matter of secondary consideration, which may either accelerate or retard, but he leaves it to be inferred that, in either case, the whirl would continue without it.

Again, a whirl originates in low latitudes in the Atlantic Ocean, over near Africa, where there are no poleward gradients; it progresses westwardly, and increases in diameter and violence; the partially condensed vapour rushes out from its top like the smoke and heated air from the flue of a great furnace, or the crater of a volcano, giving rise to a hazy cloud seen at a great distance towards the east; the wave of high pressure which precedes and surrounds such meteors is observed, while yet the air is very little disturbed at the place of observation; it arrives at the Windward Islands and then at Cuba, destroying factories and sugar plantations on the way; it passes over to Florida, and along the whole eastern coast of the United States, increasing in dimensions and violence as it goes, causing great injury to plantations and commerce; it finally ends in a great gyrating storm covering the most of the northern part of the North Atlantic Ocean. All this, this new hypothesis, now brought forward by Dr. Hann, requires us to believe originated in, and is a part of, the very gentle motions of the general circulation of the atmosphere, without any inherent power upon which the astonishing mechanical effects depend.

In order to have a gyration in the air, there must be a couple of forces to cause it, but no such couple can in any way arise from the general motions of the atmosphere, or the forces which give rise to them. And if such a gyration could arise in this way, it would have to be followed up by this couple to overcome the friction, and to continue the gyration. The general circulation of the atmosphere being once established as the result of certain forces, these same forces, according to sound principles of mechanical philosophy, can no more give rise to whirls, or any kind of local disturbances of these general motions, than the force of the sun's attraction, from which results the elliptic orbit of a planet, can cause perturbations in the motion in that orbit. Local effects must have local causes; and a cyclone can no more exist in the atmosphere, independent of any local cause, but dependent only upon the forces which give rise to the general circulation, than the motions of the satellites of Jupiter can be independent of the attraction of that planet, and depend simply upon the attraction of the sun.

As the old hypothesis required the whirl, once produced, to be followed up by counter-currents, so the new requires it to be followed up by a couple of forces, to keep it going. Scarcely anything, therefore, could be better or more appropriately said with regard to the new hypothesis, and in favour of the condensation theory of cyclones, than what Dr. Hann (*Zeitsch. für Meteorologie*, B. x., p. 82), has himself said with regard to the old hypothesis, namely:—

"No one can deny that whirls may arise from the meeting of currents at an angle, but it may be difficult, indeed impossible, to explain in this way the exclusive occurrence of gyrations from right to left in the northern, and the contrary in the southern, hemisphere. It is still more difficult to explain by this theory the fact that the whirl, once formed, progresses hundreds of miles; and meanwhile it continually draws into its motion new masses of air, overcomes a great amount of frictional resistance along the whole path, and produces powerful mechanical effects. Without a constant supply of energy it would be physically impossible—it would be the perpetual motion. To assume a continual new meeting of winds throughout in the requisite direction along the whole course of diversified wind regions, must appear to everyone, however, as a mere play of the imagination.

"The more recent theory of the origination of cyclonic motion through the deflecting force of the earth's rotation upon the air flowing in toward the minimum pressure entirely clears up the unexceptionable gyration of it from right to left in the northern hemisphere, and the contrary in the southern hemisphere. It also especially satisfies us in that it points out the source from which new energy always comes to the gyration when once originated. This comes from the fact that every large cyclone is accompanied by a rich condensation of aqueous vapour. The latent heat set free in condensation causes an accelerated ascension of the air in the cyclone, and so continually produces an underflow of the air from all sides. We now see that the gyration can advance forward, indeed that it must do so, if it continues to exist. The force which is necessary for overcoming the frictional resistance, for the drawing in of the hereto quiet air, for its powerful mechanical effects—this already is laid up in store in the air along the path over which it shall pass. It is still latent, but it is freed in being drawn in. When the cyclone does not find enough of aqueous vapour in the air, and at the same time has to overcome great frictional resistance, then must it soon come to a stand.

Since this more recent theory, in this way, gives us a conclusive physical reason of one of the most important phenomena coming into consideration in storms, I have allowed myself to designate it as the 'physical' theory."

Let us now consider the hypothesis that the maximum and minimum barometric pressures depend upon descending and ascending currents. Dr. Hann does not seem to confide wholly in the anticyclone hypothesis of areas of high pressure, though he still calls them anticyclones. He therefore devised another hypothesis to account for them—namely, that they result from an increase of pressure by the downward current which he supposes exists in these areas, and so regards them as the cause, at least mostly, and not as the effect of the high pressures. He still adheres to the old hypothesis that the zones of high pressure a little beyond the tropics in both hemispheres are caused by the crowding of the upper poleward currents into intermeridional spaces gradually becoming narrower toward the poles, and so by their being deflected down towards the earth's surface, although these high-pressure zones have long since been satisfactorily accounted for, without any mere hypothesis, upon true mechanical principles. Starting out from this hypothesis, he says that even beyond these zones there must be local obstructions and a damming up of the air in places in the higher latitudes, and a consequent deflecting of the currents down toward the earth's surface (*Zeitsch. für Meteorologie*, B. xiv., p. 39). This seems to be what is meant by the forms of motion, in the quotation above, to which is ascribed mostly the temperature relations in the barometric maximums and minimums. It does not appear, however, how the minimums of pressure can be explained by this hypothesis, for both ascending and descending currents require an increase of pressure at the bottom, where there are no lateral differences of temperature and density.

The preceding hypothesis, unlike many others, can readily be tested by means of the well-known formula showing the relation between pressure and velocity, which is based upon true and undisputed principles of mechanics. If there were a perpendicular wall around the globe on the 35th parallel of latitude, extending up to the top of the atmosphere, so that any poleward motion would have to be entirely stopped, and we suppose the upper half of the atmosphere between it and the equator to have a poleward motion toward this wall of 10 miles per hour, and that the whole is stopped, turned downward, and deflected back on the lower half of the atmosphere, the greatest increase of barometric pressure, according to the formula, which could arise from this, would be less than 0·004 of an inch. But a very small part only of the air in these high-pressure zones is stopped and turned downward, and the rest passes on to higher latitudes, so that the real effect must be very much less than this. But the observed excess of pressure in these zones is about 0·3 of an inch on the average. Hence the hypothesis could not account for the one-seventy-fifth part of it if all the kinetic energy were there converted into pressure; but considering the very small part which is so changed, it scarcely accounts for the one-thousandth part.

With regard to high-pressure areas being caused by descending currents, it would require a downward velocity of more than 170 miles per hour to cause an increase of 1 inch in the barometric pressure. The same effect would be produced by a horizontal current of that velocity if the kinetic energy were all converted into pressure by a total stoppage of the current; but where the velocity is only slightly hindered by a damming up through obstructions, the velocity would have to be many times more. Hence the hypothesis is entirely inadequate to cause even any measurable increase of barometric pressure.

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THE FLORA OF THE REVILLAGIGEDO ISLANDS.

THE somewhat peculiar flora of Lower California, as revealed by comparatively recent American explorations, aroused the curiosity of botanists concerning the probable composition of the vegetation of the Revillagigedo group of islands, situated between 18° and 19° N. lat., off the west coast of Mexico. During the spring of 1889, the United States Fish Commission steamer *Albatross* visited the two principal islands, Socorro and Clarion; and Dr. G. Vasey and Mr. J. N. Rose have just published the results of their investigations of a collection of dried plants made in these islands by Mr. C. H. Townsend, the ornithologist of the expedition. A less interesting flora could hardly be imagined, if this be a fair sample of it; but on this point the report in question affords no information whatever. Considering the distance of the islands from the nearest points of the continent, and the size of the principal island, a flora possessing some peculiarities might have been expected, and possibly the few dried plants brought away by Mr. Townsend by no means represent the flora, either as to quantity or as to quality.

Socorro is described as the largest of the group, about twenty-four miles long by nine broad, with elevations up to 2000 feet; and the position is given as 18° 43′ 14″ latitude and 110° 54′ 13″ longitude, being about 260 miles south of Cape San Lucas, Lower California, and nearly the same distance from the nearest point of the Mexican coast. Clarion, a much smaller island, in nearly the same latitude, lies about 4° to the west.

"The total number of species found on the two islands was twenty-six; eighteen are from Socorro, and twelve from Clarion Island, four of which they have in common." The sentence quoted is preceded by the statement that the flora of these islands is doubtless tropical and similar to that of Mexico; a statement that is a little ambiguous, because, although these islands are situated within the north tropic, the plants collected are mostly characteristic of warm temperate and sub-tropical regions rather than of the tropics. In this apparently poor flora, for there is no mention of the existence of any other plants besides those enumerated, are such widely-dispersed plants as *Portulaca pilosa*, *Waltheria americana*, *Tribulus cistoides*, *Dodonæa viscosa*, *Sophora tomentosa*, *Elytraria tridentata*, and *Lantana involucrata*. Ten of the others are undetermined species of common genera, and may be common species; three are described as new, one of which had been previously collected in Lower California—*Cardiospermum Palmeri*, *Perityle socorroensis*, and *Teucrium townsendii*. The Mexican *Aristolochia brevipes*, and the widely spread tropical American parasite *Phoradendron rubrum*, are also recorded as doubtful identifications, the material being too scanty to admit of certainty. This is all the information one can extract from the report; perhaps a more detailed account of the islands and their natural history may appear in some other publication connected with the expedition.

W. Botting Hemsley.

ON LOCAL MAGNETIC DISTURBANCE OF THE COMPASS IN NORTH-WEST AUSTRALIA.

AS the subject of how far the compasses of a ship, when near land, are affected by local magnetic disturbance has hitherto been more frequently one of controversy rather than a study of facts, it seems important that full publicity should be given to well-authenticated observations.

In September 1885, on board H.M. surveying-vessel *Meda*, when passing Bezout Island near Cossack, North-West Australia, a steady deflection of her compass of 30° was observed, whilst the ship was running over half a mile